

there is nothing adventitious ; the pipe is a mechanism designed to a precise end which it fulfils ; it speaks but as it must ; there is no selective power, for the hand that fashions it, ordains.

HERMANN SMITH

### Periodicity of Rainfall

In his second letter (*NATURE*, vol. x. p. 263) Governor Rawson makes the following remarks:—"Mr. Meldrum, in his letter (vol. viii. p. 547), writes, that I have 'taken 1846 and 1871 as middle maxima years [in my first paper I also took 1848], whereas 1849 and 1872 are probably more correct.' Mr. Meldrum is in error as to my having taken 1846 as a middle maximum, as a reference to my former letter will show. . . . I demur to the changes to 1849 and 1872 : to the first because, without any sufficient reason, a dry year (48.10 in.) is discarded, and a wet year (67.88 in.) is added ; and to the second, not because it affects my calculations, but because no reason is given."

In reply, I beg to observe that 1846 is either a misprint for 1848, or that in my manuscript 6 was inadvertently written for 8. This, I submit, is evident from the words immediately following the mistake, namely, "in my first letter, I also took 1848."

If Mr. Rawson supposes, or if his remarks imply, that I made 1849 a middle maximum, to avoid the small rainfall of Barbados in 1847 (48.10 in.) and at the same time to take advantage of the large fall in 1850 (67.88 in.), in order to make out a favourable case, I beg to say that he is entirely mistaken ; for long before I saw his rainfall returns, I had invariably taken 1849 as a middle maximum year. The only instance in which I took 1848 was, as I said, "in my first paper" read before the Meteorological Society of Mauritius on Oct. 10, 1872. In all subsequent papers on the subject, including one read before the Royal Society, 1849 was taken. Rightly or wrongly, therefore, the Barbados rainfall has been subjected to exactly the same treatment as that of the British Islands, the Continent of Europe, India, America, &c.

Assuming a causal connection between sun-spots and rainfall, it seemed to me that the effects, if any, would be most apparent about the times of the turning-points of the sun-spot curve, and that a comparison of the rainfall of each maximum period of three years with that of each minimum period of three years, for a considerable time and space, would be a preliminary test of the hypothesis. The difficulty was to know the exact epochs of maximum and minimum sun-spot frequency, and at the same time the rainfall for equal periods on either side of them. If we had the monthly rainfalls, and knew in what month the maximum and minimum of sun-spots occurred, it would be comparatively easy to compare the rainfalls for equal times with respect to the epochs. But there was another point to be considered, namely, that a cause requires time to produce its effect.

According to Prof. Wolf 1848.6 was a maximum epoch ; which, I presume, means that the turning-point occurred in August 1848 ; the figures, however, might mean six-tenths of a year after 1848, or August 1849.

Taking August 1848 for the maximum epoch, the strict course, in order to place the epoch at the middle of thirty-six months, would be to give the rainfall from the 6th of February 1847, to the 6th of February, 1850. But this could not be done. It was necessary to choose a *whole* year as the middle maximum year. And the reason why 1849 was chosen in preference to 1848 was, that the object being to find whether the periodic changes indicated by sun-spots had any effect upon rainfall, and time being required for a cause to produce its full effect, there was a presumption that the maximum rainfall would take place after the maximum of sun-spots, somewhat in the way in which the maximum diurnal temperature occurs, not at noon, but an hour or two after noon.

For a similar reason 1872 was taken as a middle maximum in preference to 1871.

This allowance of time for the supposed cause to produce its effect is, though apparently unintentionally, made by Mr. Rawson himself when he adopts 1844, 1856, 1860, and 1867 as middle years ; for, according to Wolf, the epochs were 1844.0, 1856.2, 1860.2, and 1867.1, that is, if I mistake not, early in each year ; so that nearly two of each of the three years taken come after the epoch, while only one of them precedes it. By taking 1849, therefore, as a middle maximum year, we come nearer to the conditions observed with respect to the other epochs than we should do by taking 1848.

Before proceeding to deduce a few results from Mr. Rawson's valuable "Report upon the Rainfall of Barbados," from 1843 to

1871, with a copy of which he has favoured me, I would remark that he has made apparently some oversights in his letter. For example, he says, with reference to a comparison of the rainfalls at Fairfield and Halton, "but the rainfall at Fairfield during the last three years . . . is 13.33 per cent. below that of Halton. Therefore 21.7 in. have to be added to the minimum average of 1843-45, which would increase the above excess of 10.6 in." But if the minimum average be increased by a percentage, would it not be well to increase also the maximum average of 1847-49 by the same percentage ? If this be done, the excess is not altered in the least.

The earliest rainfall observations at Barbados, given by Mr. Rawson, were those taken at Fairfield from 1843 to 1850, after which there is a long blank. Now, the rainfall there during that period gives the following results:—

Min. years.	Rain.	Max. years.	Rain.
1843-45.....	163.7	1848-50.....	179.7

showing an excess of 16 inches in the maximum period.

The next earliest and most complete observations are those taken at Husbands ; they commence with 1847, and have been continued without interruption. From them we get:—

Max. years.	Rain.	Min. years.	Rain.
1848-50.....	182.3	1855-57.....	188.1
1859-61.....	183.3	1866-68.....	162.8

365.6

350.9

which gives an excess of 14.7 inches on the maximum side.

The greatest number of inter-comparable observations for the longest period are those taken at the eight stations, Binfield, Henly, Husbands, Grand View, Oughtersons, Halton, Edgecumbe, and St. Ann's, from 1855 to 1868 ; and I find that they give a mean excess of 56.9 inches on the side of the years of maximum sun-spot.

I do not think that these results are *opposed* to the hypothesis which Mr. Lockyer and myself have put forward. As a matter of fact, the rainfall of Barbados, as given by Mr. Rawson from 1843 to 1868, bears out the hypothesis if we take 1849 as a middle maximum in place of 1848 ; and it is for others to judge whether the reasons that have been assigned for the change from 1848 to 1849 (not for Barbados alone, but generally) are valid.

But it may be said that the rainfall of 1871-73 was opposed to the hypothesis. I have not the rainfall for those years before me. Granting, however, that they show a very considerable diminution, the question arises whether the favourable result of twenty-six years (1843-68) are to be upset by the unfavourable results of three years (1871-73) ? Have we not in meteorology many such exceptions to well-established laws ?

The rainfall at 250 stations in different parts of the world has now been examined, and the results are so decidedly favourable that it is practically of no consequence whether the experience of Barbados is for or against the theory. I think the more the subject is examined, the more clearly will the law come out ; but we must be guided by facts, and not hesitate to discard this or any other theory when unsupported by facts.

Mauritius, Oct. 15

C. MELDRUM

### Ice-Caves

THE occurrence of snow and ice in an old mine during the month of June, mentioned by Mr. J. Clifton Ward in his interesting paper in *NATURE*, vol. xi. p. 309—to the accuracy of the greater part of which I can bear personal testimony—has a more exact parallel in the Alps than "a Swiss glacier," namely, a *glacière*. These remarkable caverns have been fully described by Mr. G. F. Browne in his able and pleasant work, "Ice-Caves of Switzerland and France ;" and briefly by myself in "The Alpine Regions." Since the publication of that book I have seen others ; and as one of these has never, I think, been described in any English work, I venture to take the opportunity of sending you a short account of it. It is in the Val d'Hérens, a short distance from Evolena, on the way to the Pic d'Arzinol, and is called the Pertuis Freiss. A slip or subsidence of part of a cliff appears to have cracked the rock and opened two joints, into one of which fissures one can descend. This is about four feet wide and generally some four yards high, the floor being a little below the level of the ground outside. The crevice comes to an end in about a dozen yards. Against the slightly sloping wall of rock rested some pendent sheets of ice, whose thickness rarely appeared to exceed three inches, and irregular patches of ice lay about the floor. The temperature of the air appeared to be a little above the freezing (unfortunately, I had

not a thermometer with me). It was a warm summer's day—July 23. The ice exhibited the usual prismatic structure, but the prisms seldom exceeded a third of an inch in diameter. I was informed that in winter it was choked up with snow. The other fissure also contained ice, but as it was less accessible, and seemed in no way different from the former, I did not enter it. The especial interest of this case is that it affords what I might call the most rudimentary type of a *glacière*; a natural ice-house, replenished every winter, and perhaps sometimes entirely cleared out during an unusually hot summer. The "Grotto" on Monte Tofana, near the Ampezzo Pass (which I have not been able to visit), is, I expect, another of this kind.

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[By a misprint "glacier" was put for *glacière* in the last paragraph of Mr. Ward's paper.—ED.]

### The Morse Code

THE following mnemonic device may be of some use to young telegraph students, and others, who wish to commit the Morse alphabet to memory. There is, I believe, a device employed in the Government schools, but it gives one so little help that I lately jotted down the subjoined scheme for my own instruction.

Let the vowels *a e i o u* and also *sh* represent the dots, and the remaining letters of the alphabet the dashes in the Morse code: the word attached to each letter will then express the signal for that letter. These words must be learnt; a task rendered easy by their commencing with or containing the letter they signify.

A . . .	at	N — .	no
B — . . .	base	O — — —	P. Q. R
C — . — .	cave	P . — . .	Apps
D — . . .	die	Q — — —	Q. Q. E. D
E . . .	E	R . . .	are
F . . . .	safe	S . . .	ass
G — — .	Gnu	T —	T
H . . . .	hush	U . . —	Usk
I . . .	is	V . . . .	Asov
J — — — —	Ujji	W — —	awl
K — . —	kit	X — . . .	Faux
L . . . .	aloe	Y — . . . .	yawl
M — — —	my	Z — — . .	zwei

A few of the letters, e.g. J (the word for which might be regarded as a new way of spelling Ujji), O, and Q, present a little difficulty, which some of your readers may lessen. As it is, these exceptional cases are so quickly impressed on the memory that the code thus learnt can be written in a surprisingly short time, and read soon afterwards. It is hardly possible the plan here suggested can be new, yet, as I have not met with anything similar, I venture to send it to you for publication.

W. F. BARRÉTT

### The Micrographic Dictionary—Pollen Grains

AT present I have to do with the "Micrographic Dictionary" and the two other works mentioned in my letter printed in NATURE, vol. xi., p. 286. If the pollen grains of *Mimulus moschatus* are variable (as now stated by Mr. Cooke on the authority of Dr. Mohl), how is it that the figures and descriptions in the books mentioned are all alike? There is no variability here, but wonderful sameness both in illustrations and letter-press.

As the accuracy of my first simple observation has been called in question, I will add another. In the "Micrographic Dictionary," Pl. 32, Fig. 28, is given the pollen of *Sonchus palustris*. This, like that of the *Mimulus*, is totally wrong, the reticulation is by no means correct, and the abundant spines with which this pollen grain is clad (so common in the Composite) are totally omitted. Now, on turning to the Rev. J. G. Wood's book, Pl. 3, Fig. 24, this erroneous figure is reproduced with incorrect reticulation and no spines, and on referring to Mr. Cooke's work, Pl. 2, Fig. 6, the same errors are again perpetrated.

W. G. SMITH

### OUR ASTRONOMICAL COLUMN

ζ<sup>1</sup> AND ζ<sup>2</sup> RETICULI.—These stars of about the sixth magnitude appear to offer a similar instance of large and nearly equable proper motion to the well-known one

afforded by 36 Ophiuchi and 30 Scorpii, which was first pointed out by Bessel in the "Fundamenta Astronomiæ." If we compare Lacaille's positions (taking them from the reduced catalogue published by the British Association) with those given by the late Capt. Jacob from the Madras observations 1853-57, we find with the Pulkova precessions—

	R. A.	Secular Proper Motion,		Direction of
		N. P. D.	Arc of great circle.	motion.
$\zeta^1$ ...	+ 237''·5	− 74''·9	130''·3	54°·9
$\zeta^2$ ...	+ 238''·7	− 79''·6	133''·8	53°·4

The introduction of Brisbane's places would only modify the above figures in a trifling degree.

When competent observers in the southern hemisphere are provided with heliometers for research on stellar parallax, there will be no lack of objects to occupy their attention, and we may expect most important results from such investigations.

THE BINARY STAR η CASSIOPEÆ.—We may very soon be able to make a fair approximation to the orbit of this double star, and so, with Mr. Otto Struve's value for the annual parallax, form some idea of the real dimensions and mass of the system, as is already the case with α Centauri and 70 Ophiuchi. An orbit given by Mr. Powell, of Madras, in vol. xxi. of *Monthly Notices*, R. A. S., is probably vitiated by typographical error or errors. Struve's parallax is 0''·154 ± 0''·045.

THE BINARY STAR α CENTAURI.—According to Mr. Powell's last elements, which are founded on measures up to 1870 inclusive, the components, at the present time, are nearly at their minimum apparent distance (1''·2), and the angle of position is advancing at the rate of 10° monthly. It may be hoped this fine object is receiving due attention from astronomers in the southern hemisphere at this critical period of the revolution. There would appear to be no probability of such difficulties attending observations at the passage of the peri-astron as those presented by γ Virginis in 1836, so far at least as can be judged from the measures to 1870.

RED STARS.—Amongst the red stars notified by the late M. Chacornac, is one which he estimated between the seventh and eighth magnitude, and of which he says, "éclat terne et nebuleux." The position assigned identifies the star with No. 1172 of Rümker's Catalogue, whence for the commencement of the present year its right ascension is 4h. 16m. 16s., and polar distance 67° 19'·7. Rümker calls it a sixth magnitude, and Argelander (Durchmusterung) an eighth. Although different eyes will not always agree in estimations of brightness of the ruddy stars, there appears here to be a suspicion of variable light. Another of Chacornac's isolated red stars he himself indicates as variable. It is Oeltzen 21356, called 6 mag. by Lalande (No. 41453), 5·6 by Argelander, 5 in the Washington Zone, 1848, July 24; while Chacornac remarks, "sometimes brighter and sometimes fainter than a star of the seventh magnitude near it," which is probably Oeltzen 21386. Position for 1875, R. A. 21h. 17m. 5s.; P. D., 111° 22'·7. Neither of these stars is in Schjellerup's Catalogue, but that list is very far from being a complete one.

ENCKE'S COMET.—The extreme faintness of this comet at the present appearance is attracting the attention of astronomers who have had most experience of the circumstances of previous returns. Last week we quoted the remark of M. Stéphan on this subject, and we learn from him that he was using a newly polished mirror in the great Foucault telescope of the Observatory of Marseilles. In 1868 and 1871 the comet's appearance was very similar to what it had been in previous years under analogous conditions. In discussing the probability of any real change in the comet's constitution, it may, however, be well to bear in mind that in the year 1842, when the peri-